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ROYAL ASTRONOMICAL SOCIETY.

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No. 3.

WILLIAM HUGGINS, ESQ., F.R.S., President, in the Chair.

The Rev. William H. Allen, 3 Kelly Street, Kentish Town Road, N.W.;

T. H. E. C. Espin, Esq., Wallasey Rectory, Birkenhead;

The Rev. David Fleming, Coxhoe Vicarage, Co. Durham;

F. R. Janisch, Esq., C.M.G., Governor of St. Helena;

Lieut.-Col. H. S. Knight, Army and Navy Club, S.W.;

Albert Marth, Esq., 66 Lambeth Road, S.W.;

George Francis Riddiford, Esq., Barnwood Lodge, near Gloucester;

The Rev. Philip R. Sleeman, Clifton, Bristol; and

The Rev. G. E. Watts, Kensworth Vicarage, Dunstable;

were balloted for, and duly elected Fellows of the Society.

Mr. D. Gill's Expedition to Ascension. Report, December 3, 1877.

Since my last report the weather has been much more unfavourable than any I have yet experienced here.

With an unceasing watch up to December 2, only three evening sets and one morning set of observations of *Melpomene* could be secured, and all were more or less imperfect.

The first half of opposition thus lost was, both as to distance of planet from the Earth and choice of comparison stars, far better than the second half; the weather shows no sign of improvement, and even under favourable circumstances it would be impossible now to secure what I should consider a satisfactory series of observations from which to find corrections of the planet's tables, and to deduce a value of the parallax. I have therefore decided not to attempt further observations of *Melpomene*.

I propose to devote the remainder of my stay to latitude and longitude determinations, and such other work as I can

accomplish before Christmas. After that I propose to dismantle the Observatory, pack the instruments, and return by the January mail. I forward along with this report a copy of observations in the triangulation of the *Mars* stars of comparison. I am happy to think that the main objects of the expedition are thus accomplished.

*Mars Bay, Ascension Island,
December 3, 1877.*

Note sur les mouvements réels des étoiles dans l'espace.

By Señor V. Ventosa.

(Communicated by W. H. M. Christie.)

L'un des problèmes les plus intéressants qu'ait suscités l'application du spectroscope à l'étude des corps célestes est, sans doute, la détermination des mouvements des étoiles dans la direction du rayon visuel, au moyen du déplacement observé des raies de leur spectre. Plusieurs astronomes ont énoncé l'idée d'une relation, susceptible d'être exprimée par l'analyse mathématique, et qui nécessairement doit exister entre le mouvement *radial*, ainsi obtenu, et le mouvement angulaire apparent d'une étoile, puisque les deux ne sont que les composants du mouvement relatif de celle-ci dans l'espace. La Société Royale Astronomique de Londres a donné son opinion autorisée sur le nouveau procédé, "dont les résultats positifs—dit-elle—lorsqu'ils seront combinés avec les mouvements propres ordinairement observés dans les étoiles, nous fourniront des renseignements sur la vraie constitution du ciel." On a même indiqué quelques applications des mouvements radiaux à divers problèmes, tels que la détermination de la parallaxe des étoiles doubles; mais encore je ne connais aucun travail où la relation mentionnée ait été envisagée d'une manière générale.

Un Mémoire qui paraîtra bientôt contient les premiers résultats des recherches que j'ai poursuivies dans cette voie depuis Avril 1873, autant que les circonstances me l'ont permis, et dont je me propose de faire ici un court résumé.

Dans le mouvement relatif d'une étoile, le seul que nous puissions connaître par l'observation directe, et en supposant d'ailleurs, comme d'habitude, l'observateur placé au centre du soleil, il faut naturellement considérer les mouvements absolus de ces deux astres, en grandeur et en direction, leur distance mutuelle, et les mouvements radial et angulaire observés de l'étoile, ainsi que son lieu apparent sur la sphère céleste. Quelques-unes de ces quantités devant être exprimées en unités linéaires, tandis que les autres le seront en mesure angulaire, j'ai tâché, pour plus de clarté, de distinguer les premières par les caractères de notre alphabet, et les dernières par des lettres

grecques, en y conservant du reste, lorsqu'il a été possible, la notation établie et sanctionnée par d'éminents astronomes.

Voici, d'abord, comment j'obtiens les équations fondamentales. Le triangle sphérique formé par le pôle de l'équateur céleste, le point du ciel (apex) vers lequel se dirige notre soleil, et une étoile, fournit les expressions connues :

$$\left. \begin{aligned} \sin \psi \sin \chi &= \sin (\alpha - \alpha_0) \cos \delta_0, \\ \cos \psi \sin \chi &= \cos (\alpha - \alpha_0) \cos \delta_0 \sin \delta - \sin \delta_0 \cos \delta, \\ \cos \chi &= \cos (\alpha - \alpha_0) \cos \delta_0 \cos \delta + \sin \delta_0 \sin \delta, \end{aligned} \right\} \quad (A)$$

où α , δ , α_0 , δ_0 , sont respectivement l'ascension droite et la déclinaison de l'étoile, et du sommet parallactique ; $180^\circ - \psi$ l'angle à l'étoile, et χ la distance de celle-ci au sommet.

Le mouvement propre angulaire ρ , déterminé par les mouvements en ascension droite et en déclinaison, et projeté sur le cercle parallactique χ , donne aussi, comme l'on sait,

$$\left. \begin{aligned} \rho \sin \psi_1 &= \Delta \alpha \cos \delta, & \beta &= \rho \sin (\psi - \psi_1), \\ \rho \cos \psi_1 &= \Delta \delta, & \gamma &= \rho \cos (\psi - \psi_1), \end{aligned} \right\} \quad (B)$$

où γ est la projection du mouvement et β la composante perpendiculaire.

Considérons maintenant les relations dans l'espace. Soient deux positions successives du soleil et de l'étoile aux instants t et t' , suffisamment rapprochés entre eux pour que l'on puisse, dans l'intervalle, prendre comme rectilignes les déplacements élémentaires des deux astres. Concevons en outre un système de coordonnées rectangulaires, dont, pour plus de simplicité, nous placerons l'origine en coïncidence avec la position initiale du Soleil, et ensuite nous en ferons passer l'un des axes par le deuxième lieu du même astre, et l'un des plans (le *plan parallactique*) par la première position de l'étoile. Ordinairement les quatre points considérés ne seront pas situés dans le même plan ; donc si on les projette sur les axes coordonnés, on trouvera facilement les équations suivantes :

$$\left. \begin{aligned} r \cos \chi + n \cos \zeta \cos (\chi + \theta) &= m + r' \cos \beta \cos (\chi + \gamma), \\ r \sin \chi + n \cos \zeta \sin (\chi + \theta) &= r' \cos \beta \sin (\chi + \gamma), \\ n \sin \zeta &= r' \sin \beta, \end{aligned} \right\} \quad (C)$$

en appelant m et n les éléments rectilignes des trajectoires du Soleil et de l'étoile ; r et r' les distances entre ces deux corps aux instants t et t' ; ζ l'angle que fait la direction de l'étoile dans l'espace avec sa projection sur le plan parallactique ; et θ l'angle formé dans ce plan par la même projection et le rayon visuel initial.

Ces formules sont rigoureuses, mais β et γ étant toujours deux angles très-petits, on peut en déduire d'autres suffisamment approchées et qui offriront plus d'utilité. Pour cela on y mettra $\sin \beta = \beta \sin 1''$, $\cos \beta = 1$, $\sin \gamma = \gamma \sin 1''$, $\cos \gamma = 1$; et après

des transformations très-simples, si l'on y fait $r' - r = s$, $r' = r$ (dans les termes où r' multiplierait soit γ , soit β), et, enfin, $r \sin i' = p$, il en résultera :

$$\left. \begin{aligned} n \cos \zeta \cos (\chi + \theta) &= -p\gamma \sin \chi + s \cos \chi + m, \\ n \cos \zeta \sin (\chi + \theta) &= p\gamma \cos \chi + s \sin \chi, \\ n \sin \zeta &= p\beta. \end{aligned} \right\} \quad (D)$$

La quantité indiquée par s (initial du *spectrum*) n'est que le mouvement radial observé, et p , qu'on pourrait appeler *distance réduite*, me paraît une sorte de mesure très-convenable pour les grandeurs sidérales. Les équations (D) peuvent encore recevoir la forme plus simple que voici :

$$\left. \begin{aligned} n \cos \zeta \cos \theta &= s + m \cos \chi, \\ n \cos \zeta \sin \theta &= p\gamma - m \sin \chi, \\ n \sin \zeta &= p\beta. \end{aligned} \right\} \quad (E)$$

Dans les deux systèmes (D) et (E) on doit exprimer m , n , p , s , en rayons moyens de l'orbite terrestre : β et γ , qui ne représentent plus que des nombres abstraits, doivent l'être en secondes d'arc. Nous avons pris l'année pour unité de temps, en sorte que si s est donné en milles anglaises par seconde, il faudra le multiplier par $\frac{t-t'}{a} = 0.3431$, a étant la distance moyenne de la Terre au Soleil.

Lorsqu'il s'agit de comparer entre eux les mouvements de deux étoiles dans l'espace, il faut employer de nouvelles expressions. Le même triangle sphérique auparavant considéré donne aussi les équations suivantes :

$$\left. \begin{aligned} \sin \omega \sin \chi &= \sin (\alpha - \alpha_0) \cos \delta, \\ \cos \omega \sin \chi &= -\cos (\alpha - \alpha_0) \cos \delta \sin \delta_0 + \sin \delta \cos \delta_0, \\ \cos \chi &= \cos (\alpha - \alpha_0) \cos \delta \cos \delta + \sin \delta \sin \delta_0, \end{aligned} \right\} \quad (F)$$

où ω est l'angle au sommet parallactique. Menons par le centre du Soleil des parallèles aux directions *actuelles* des trajectoires des étoiles : chaque parallèle coupera la sphère céleste en un point, qui pourra être regardé comme une nouvelle étoile. En appliquant, donc, les mêmes formules (F) à ce point, si l'on y distingue avec un accent les quantités analogues, on trouvera :

$$\left. \begin{aligned} \sin (\alpha' - \alpha_0) \cos \delta' &= \sin \omega' \sin \chi', \\ \cos (\alpha' - \alpha_0) \cos \delta' &= -\cos \omega' \sin \chi' \sin \delta_0 + \cos \chi' \cos \delta_0, \\ \sin \delta' &= \cos \omega' \sin \chi' \cos \delta_0 + \cos \chi' \sin \delta_0. \end{aligned} \right\} \quad (G)$$

D'après cela, ω' et χ' une fois connues, on obtiendrait les coordonnées α' et δ' de la trace de la trajectoire. Or, il n'est pas difficile de voir que si l'on projette cette trace sur la corde parallactique de l'étoile, on aura finalement :

$$\left. \begin{aligned} \sin(\omega - \omega') \sin \chi' &= \sin \zeta, \\ \cos(\omega - \omega') \sin \chi' &= \cos \zeta \sin(\chi + \theta), \\ \cos \chi' &= \cos \zeta \cos(\chi + \theta). \end{aligned} \right\} \quad (H)$$

Mon Mémoire contient quelques exemples numériques détaillés de l'emploi des formules précédentes. Je ne peux que mettre ici les résultats obtenus avec l'étoile α *Lyræ*. En prenant pour données les nombres :

$$\begin{array}{lll} \alpha_0 = 259^\circ 52' & \alpha = 277^\circ 58' & \Delta \alpha = + 0'' 255 \\ \delta_0 = + 32^\circ 29' & \delta = + 38^\circ 39' & \Delta \delta = + 0'' 28 \\ m = 1,623 & \pi = 0''.180 & s = - 36.6 \text{ (milles)} \end{array}$$

il viendra successivement

$$\begin{array}{llll} \psi_1 = 35^\circ 29' & \rho = 0''.344 & s = - 12,560 & \theta = 174^\circ 24' \quad \alpha' = 93^\circ 49 \\ \psi = 72^\circ 44' & \beta = + 0''.208 & p = 5.550 & \zeta = + 5^\circ 58' \quad \delta' = - 31^\circ 12 \\ \chi = 15^\circ 56' & \gamma = + 0''.274 & n = 11,112 & \chi' = 168^\circ 5' \\ \omega = 62^\circ 9' & & & \omega' = 272^\circ 25' \end{array}$$

Il s'ensuit, qu' α *Lyræ* se mouvrait dans l'espace en direction presque opposée et avec une vitesse beaucoup plus grande que le Soleil. Ces conclusions supposent, toutefois, exactement connues toutes les données numériques du problème, qui en tel cas serait déterminé. Mais, en réalité, on ignore la parallaxe de la plupart des étoiles, et par conséquent, les valeurs de r et p ; et celles qu'on a trouvées par l'observation ne sont pas jusqu'à présent très-sûres. En outre, les distances relatives basées en diverses hypothèses sur la grandeur apparente des étoiles, ainsi que sur leurs mouvements propres angulaires, sont encore l'objet de continuel débats. D'ailleurs, si l'on peut admettre comme assez connue la direction du mouvement du Soleil, ce n'est pas de même de la vitesse linéaire m de ce mouvement, qui dépend des mêmes distances hypothétiques; en sorte que dans nos formules il faudrait considérer m et p comme des inconnues, avec n , ζ et θ . Cependant, d'une discussion qu'il n'est pas possible de reproduire ici, il résulte que m , n et p devant être des quantités essentiellement positives et leurs valeurs numériques comprises entre des limites convenables, ces conditions, et d'autres provenant de la structure même des équations fondamentales, permettront, dans la plupart des cas, de déterminer approximativement la région de l'espace vers laquelle une étoile se ment, au moyen de ses mouvements propres observés, radial et angulaire.

Un chapitre du Mémoire est consacré à la question importante du mouvement du Soleil. En effet, tous les procédés employés jusqu'à ce jour pour cette recherche découlent de quelques-unes des formules précédentes, et celui habilement développé par MM. Airy et Dunkin peut être déduit d'une manière simple des deux dernières équations (E), en substituant χ , β et γ dans (A) et (B). La première équation (E) me paraît très-convenable

pour la solution du même problème, puisqu'elle ne contient que le mouvement radial et ne dépend pas des distances des étoiles. Après des substitutions faciles elle prend la forme :

$$s' = s + x_0 \cos \alpha \cos \delta + y_0 \sin \alpha \cos \delta + z_0 \sin \delta, \quad (I)$$

où

$$x_0 = m \cos \alpha_0 \cos \delta_0, \quad y_0 = m \sin \alpha_0 \cos \delta_0, \quad z_0 = m \sin \delta_0, \quad \text{et} \quad s' = n \cos \zeta \cos \theta.$$

s' n'est que la partie du mouvement radial due au mouvement réel de l'étoile, ou, ce qui revient au même, la projection de ce mouvement sur le rayon visuel.

En supposant, comme à l'ordinaire, que les mouvements particuliers des étoiles ont lieu indifféremment dans tous les sens possibles, je considère s' comme une erreur accidentelle qui doit disparaître des équations normales. Ensuite j'examine l'effet des erreurs constantes dans la détermination de α_0 , δ_0 et m , et je fais séparément l'application du procédé aux séries numériques des mouvements radiaux trouvés à Greenwich et par Mr. Huggins. Les résultats ainsi obtenus sont, à mon avis, remarquables, malgré le petit nombre des étoiles employées, leur inégale répartition sur la sphère céleste, et l'existence possible d'erreurs systématiques dans les observations. Je ne crois pas, pourtant, que les anciennes méthodes doivent être abandonnées; la comparaison de leurs résultats avec ceux que donne la méthode que j'ose proposer pouvant, peut-être, conduire à nous donner quelques renseignements sur le mouvement d'ensemble du système sidéral.

Observatoire de Madrid,
le 20 Décembre 1877.

On the Distribution of the Fixed Stars in Space.

By Sidney Waters, Esq.

In Mr. Proctor's Chart of all the stars in Argelander's maps, we have an accurate representation of the distribution of the stars of the northern heavens down to the ninth magnitude; but we have no illustration of a similar character for the southern heavens. There does not indeed as yet exist sufficient data for the construction of a chart to correspond to Mr. Proctor's; but I thought that it might be worth while, in the meantime, to utilise as far as possible the results of Sir John Herschel's star gauges given in his Cape of Good Hope observations.

I have therefore, in the chart now laid before the Society, represented on an equal surface projection these results in as accurate a manner as possible. The map was divided to 20 minutes in R.A. and 3° in P.D., and the results of the gauges were figured in their proper places, the areas of the spaces were

calculated, and the average of the gauges falling on each space multiplied by its area, the result being dotted as nearly as possible equally over the space. Nearly all the spaces were covered in this way; but in those cases where no gauges fell the average of the surrounding spaces was taken. It will be understood that the number of stars marked in the map is very small in comparison with the number which actually exist, but they represent with fair accuracy the state of distribution over the southern heavens down to the faintest stars visible in Sir John Herschel's 18 $\frac{1}{4}$ -inch Reflector. The stars visible to the naked eye were afterwards added.

The teaching of the chart does not confirm the results of Mr. Stone's investigations published in the *Monthly Notices* of last March. The great massing of stars in parts of the Milky Way, its complicated form, and the sudden decrease of stars at its borders are facts not consistent with the view that we are merely looking through an immense depth of stars, or that distance is the principal reason of their faintness. It is not denied that brightness is an important factor in the determination of stellar distances; but the point contended for is, that it is not a sufficient basis upon which to found any theory of the construction of the heavens.

With regard to the list of star parallaxes which Mr. Stone gives, he states that "we have here a proved range of stellar distance in at least the proportion of 1 to 10." The weight of this illustration will be properly estimated when it is remembered that both the first and the last on the list (*α Centauri* and *Capella*) are stars of the first magnitude, and that therefore it may with equal force be said that we have a proved range of dimension in at least the same proportion. Again, with regard to Mr. Stone's comparison of Argelander's numbers with the numbers computed on the assumption that the stars are distributed with an approximate uniformity in space, there is an excess in the observed number of ninth magnitude stars of 40,000 over the requirements of the theory, the numbers being 142,000 and 102,780. But it is necessary to take into account the fact that Argelander's number is considerably below the truth, owing to the omissions which inevitably occur in counting faint stars. The discrepancy will therefore have to be considerably increased. Notwithstanding this, Mr. Stone considers that it may be altogether disregarded. I do not think that it will be so readily conceded that a difference of at least one-third of the number involved may be disposed of so lightly.

The points, therefore, that I have to urge are, that the fainter stars increase at a far greater rate than would be the case if Mr. Stone's view were correct, and that the theory that the stars are distributed with any uniformity, either as to space or dimension, is not in accordance with the teaching of the chart which I now beg to lay before the Society.

Ephemeris for Determining the Positions of the Satellites of Uranus, 1878.

By A. Marth, Esq.

o ^b G.M.T. 1878.	Ariel.		Umbriel.		Titania.		Oberon.	
	long. °	diff. °	long. °	diff. °	long. °	diff. °	long. °	diff. °
Jan. 25	103° 99		358° 25		353° 63		288° 69	
		714° 17		434° 33		206° 73		133° 67
30	98° 16		72° 58		200° 36		62° 36	
		'16		'32		'73		'67
Feb. 4	92° 32		146° 90		47° 09		196° 03	
		'15		'32		'72		'67
9	86° 47		221° 22		253° 81		329° 70	
		'15		'31		'73		'66
14	80° 62		295° 53		100° 54		103° 36	
		'14		'31		'72		'66
19	74° 76		9° 84		307° 26		237° 02	
		'14		'31		'72		'66
24	68° 90		84° 15		153° 98		10° 68	
		'13		'30		'72		'66
Mar. 1	63° 03		158° 45		0° 70		144° 34	
		'13		'30		'71		'67
6	57° 16		232° 75		207° 41		278° 01	
		'12		'30		'72		'66
11	51° 28		307° 05		54° 13		51° 67	
		'12		'30		'72		'66
16	45° 40		21° 35		260° 85		185° 33	
		'12		'29		'72		'67
21	39° 52		95° 64		107° 57		319° 00	
		'11		'30		'72		'66
26	33° 63		169° 94		314° 29		92° 66	
		'11		'29		'73		'67
31	27° 74		244° 23		161° 02		226° 33	
		'11		'30		'72		'67
Apr. 5	21° 85		318° 53		7° 74		0° 00	
		'11		'30		'73		'67
10	15° 96		32° 83		214° 47		133° 67	
		'11		'29		'73		'68
15	10° 07		107° 12		61° 20		267° 35	
		'11		'30		'73		'68
20	4° 18		181° 42		267° 93		41° 03	
		'11		'30		'73		'68
25	358° 29		255° 72		114° 66		174° 71	
		'11		'31		'74		'68
30	352° 40		330° 03		321° 40		308° 39	
		714° 11		434° 30		206° 74		133° 69
May 5	346° 51		44° 33		168° 14		82° 08	

These longitudes of the satellites are reckoned in their orbits from the points where they are at their greatest northern elongations. Their position-angles p_0 at these points, and the logarithms of the semi-axes a and b of the apparent orbits are the following:—

oh G.M.T. 1878.	p_0	Ariel.		Umbriel.		Titania.		Oberon.	
		log. a	log. b	log. a	log. b	log. a	log. b	log. a	log. b .
Jan. 25	8 ^o 13	1 ^o 1812	0 ^o 6528	1 ^o 3252	0 ^o 7968	1 ^o 5401	1 ^o 0117	1 ^o 6663	1 ^o 1379
30	8 ^o 19	1 ^o 1819	0 ^o 6584	1 ^o 3259	0 ^o 8024	1 ^o 5408	1 ^o 0173	1 ^o 6670	1 ^o 1435
Feb. 4	8 ^o 24	1 ^o 1825	0 ^o 6639	1 ^o 3265	0 ^o 8079	1 ^o 5414	1 ^o 0228	1 ^o 6676	1 ^o 1490
9	8 ^o 30	1 ^o 1829	0 ^o 6693	1 ^o 3269	0 ^o 8133	1 ^o 5418	1 ^o 0282	1 ^o 6680	1 ^o 1544
14	8 ^o 35	1 ^o 1831	0 ^o 6746	1 ^o 3271	0 ^o 8186	1 ^o 5420	1 ^o 0335	1 ^o 6682	1 ^o 1597
19	8 ^o 41	1 ^o 1831	0 ^o 6796	1 ^o 3271	0 ^o 8236	1 ^o 5420	1 ^o 0385	1 ^o 6682	1 ^o 1647
24	8 ^o 47	1 ^o 1829	0 ^o 6843	1 ^o 3269	0 ^o 8283	1 ^o 5418	1 ^o 0432	1 ^o 6680	1 ^o 1694
Mar. 1	8 ^o 53	1 ^o 1825	0 ^o 6887	1 ^o 3265	0 ^o 8327	1 ^o 5414	1 ^o 0476	1 ^o 6676	1 ^o 1738
6	8 ^o 59	1 ^o 1819	0 ^o 6927	1 ^o 3259	0 ^o 8367	1 ^o 5408	1 ^o 0516	1 ^o 6670	1 ^o 1778
11	8 ^o 64	1 ^o 1812	0 ^o 6963	1 ^o 3252	0 ^o 8403	1 ^o 5401	1 ^o 0552	1 ^o 6663	1 ^o 1814
16	8 ^o 69	1 ^o 1803	0 ^o 6994	1 ^o 3243	0 ^o 8434	1 ^o 5392	1 ^o 0583	1 ^o 6654	1 ^o 1845
21	8 ^o 74	1 ^o 1792	0 ^o 7021	1 ^o 3232	0 ^o 8461	1 ^o 5381	1 ^o 0610	1 ^o 6643	1 ^o 1872
26	8 ^o 79	1 ^o 1780	0 ^o 7042	1 ^o 3220	0 ^o 8482	1 ^o 5369	1 ^o 0631	1 ^o 6631	1 ^o 1893
31	8 ^o 83	1 ^o 1766	0 ^o 7059	1 ^o 3206	0 ^o 8499	1 ^o 5355	1 ^o 0648	1 ^o 6617	1 ^o 1910
Apr. 5	8 ^o 86	1 ^o 1751	0 ^o 7070	1 ^o 3191	0 ^o 8510	1 ^o 5340	1 ^o 0659	1 ^o 6602	1 ^o 1921
10	8 ^o 89	1 ^o 1735	0 ^o 7076	1 ^o 3175	0 ^o 8516	1 ^o 5324	1 ^o 0665	1 ^o 6586	1 ^o 1927
15	8 ^o 91	1 ^o 1717	0 ^o 7077	1 ^o 3157	0 ^o 8517	1 ^o 5306	1 ^o 0666	1 ^o 6568	1 ^o 1928
20	8 ^o 93	1 ^o 1699	0 ^o 7072	1 ^o 3139	0 ^o 8512	1 ^o 5288	1 ^o 0661	1 ^o 6550	1 ^o 1923
25	8 ^o 95	1 ^o 1681	0 ^o 7063	1 ^o 3121	0 ^o 8503	1 ^o 5270	1 ^o 0652	1 ^o 6532	1 ^o 1914
30	8 ^o 95	1 ^o 1662	0 ^o 7048	1 ^o 3102	0 ^o 8488	1 ^o 5251	1 ^o 0637	1 ^o 6513	1 ^o 1899
May 5	8 ^o 95	1 ^o 1642	0 ^o 7029	1 ^o 3082	0 ^o 8469	1 ^o 5231	1 ^o 0618	1 ^o 6493	1 ^o 1880

These values are to be interpolated for the times for which the positions of the satellites are required. The position-angles p and distances s are then found by means of the equations

$$s \sin (p_0 - p) = b \sin \text{long.}$$

$$s \cos (p_0 - p) = a \cos \text{long.}$$

Note on the Satellites of Mars and Saturn.

By A. A. Common, Esq.

The following observations of the satellite of *Mars* were made with an 18-inch silvered glass reflector, with powers 170 and 272. In all cases the full aperture was used under favourable atmospheric conditions. The satellite was always fairly visible when the planet was hidden behind a bar in the field. On the 15th and 16th of September it was seen well, with the planet, at times. The difficulty of getting measures of position under these circumstances was got over by the use of a modification of the old slipping piece, which is to be recommended for similar observations.

This modification consists in placing the slipping piece on the position-circle, and the micrometer on that; thus the movements of the slipping piece are always, after adjustment, parallel to the horizontal and vertical wires of the micrometer.

The vertical wires of the double micrometer were taken out and two thin bands of steel put in (leaving the position or horizontal wire in). In using this arrangement, the zero being found, the position wire is placed upon or beside the satellite, with the planet hidden by the bars; then, by giving to the micrometer the motion parallel to this wire, the place of the wire with regard to the centre of the planet can be seen without altering its relation to the satellite, and, by alternately looking at both, a fair measure may be got; always subject to the error of estimation of the centre of the planet.

The colour of the satellite was distinctly the same as that of the brighter portions of the body of the planet, and had not the bright appearance of the inner satellites of *Saturn*.

It was looked for while nearer (visibly) to the planet, but not seen.

On the 15th of September, at 11^h, while looking at the satellite, a very faint point was seen about 20'' north, very much fainter than the satellite; in fact, barely visible in the glare. This was found next evening to be a very small star, about 120° position-angle and 65'' or 70'' distant from a pair of about 9th magnitude stars preceding the planet (on the 15th).

The inner satellite was looked for, but possibly only glimpsed on October 17, when the position angle was 232° 30', and dis. est. 15''.

The observations of the satellites of *Saturn* were made with the same instrument. Reducing the aperture brought out details on the body of the planet, particularly on October 26, when the shadow of the planet was well seen on the north following portion of the ring, extending nearly to the width of the space between the limb and the inner edge of the ring. *Hyperion* was frequently seen, though only once measured. *Enceladus* could not be seen when very near the edge of the ring. *Mimas* was never distinctly seen, though frequently looked for. The shadow of *Titan* was seen on October 22, at 9.55, when it had passed central conjunction by about 5'', and on November 23, when it had just got on at 5.45, clouds preventing complete observations:—

Satellite of Mars.

Date 1877.	G.M.T. h m	Dis.	Pos. ,	Value 1 to 5.
Sept. 11	9 52		270	Est.
15	10 15		253 48	1
"	10 20		252 44	1
"	10 41		251 2	2
"	10 47		250	1

Date 1877.	G.M.T. h m	Dis.	Pos.	Value 1 to 5.
Sept. 15	10 50		250 40	1
"	12 43		244 28	1
"	12 51		240 40	3
"	13 5		243	1
16	12 43		273	1
"	12 47		269	1
"	12 50		273 30	3
"	12 57		270	3
"	13 5	68".2		3
"	13 44		266 4	2
"	13 50	72".1		3
"	13 58		263 4	2
"	14 0		263 36	3
"	14 5	75".6	264 8	2
21	8 34		62 8	1
24	8 49		240 30	3
"	9 8		236 30	1
28	11 23		111	1
29	9 28		244 30	2
"	9 33		245 45	3
"	9 38		235 35	3 with N. Limb.
"	9 42		251 15	3 with S. Limb.
"	9 45		254 0	3 with S. Limb.
"	9 48		232 30	3 with N. Limb.
"	10 29		242	2
"	10 36		239 3	2
30	10 28		269 30	3
"	10 38		267 30	2
"	10 52		267 0	1
"	11 0		266	1
Oct. 16	9 55		67	1
"	10 0		67 16	1

Satellites of Saturn.

1877.	G.M.T. h m	
Sept. 19	12 35	Conjunction of <i>Titan</i> with preceding limb of planet.
Oct. 6	7 53	Inferior conjunction of <i>Titan</i> with centre of planet.
		<i>Titan</i> 3" (est.) south.
11	11 0	<i>Hyperion</i> —position 92°, distance 220".
16	7 0	<i>Japetus</i> , about 6" from conjunction with preceding edge of ring.

		G.M.T.	
	^h	^m	
1877.			
Oct. 16	9	43	Nearly "up" to conjunction.
	10	15	"Up."
	10	20	Certainly "up"; distance from plane of ring 48".8.
17	7	30	<i>Japetus</i> about half-way between limb and following edge of ring.
	8	45	<i>Japetus</i> 41".6 distance from plane of ring.
	9	35	Nearly "up"—observation stopped; conjunction would not take place later than 9 ^h 55 ^m .
Nov. 25	7	5	<i>Japetus</i> 10" past conjunction with centre of <i>Saturn</i> .
<i>Eaton Rise, Faling,</i>			
<i>January 1878.</i>			

Note on the Transit of the Shadow of Titan over Saturn, on 1877, December 25. By Lord Lindsay.

Dr. R. Copeland observing with the 15-inch refractor (reduced to 10-inch), power about 200.

^h	^m	G.M.T.	
5	36		Certainly not up.
	42		" "
	44		" "
	46		Not up.
	48		Barely up.
	50		Up.
	53		Still apparently central.
	55		" "
5	56.5		Scarcely past.
6	0.0		Very slightly past (very good definition).
6	2		Very slightly past.
6	5		Certainly past.
6	8		" "
6	12		Far past.

The following pairs may be combined :—

^h	^m		^h	^m		^h	^m	
5	46	Not up.	5	48	Barely up.	5	50	Up.
6	5	Cert. past.	6	0	Very slightly past.	5	56.5	Scarcely past.
5	55.5		5	54		5	53.3	

Say central passage 5^h 54^m.3 with limits not exceeding 5^h 48^m to 6^h 0^m.

On the whole the definition was very bad. The following measures were taken with great difficulty:—

	h	m	"
Shadow south of axis of ring at	5	45	3 34
		47	3'47
	5	52	4'50
	6	1	2'15
		7	3'43
		14	2 91
	6	17	2'78'
Means	6	04	3'23

Mr. Lohse observing with the 6-inch Simms made the shadow

Certainly not up at $\begin{matrix} h & m \\ 5 & 43 \end{matrix}$ G.M.T.

Certainly past at $\begin{matrix} 6 & 2\frac{1}{2} \end{matrix}$

and Central at $\begin{matrix} 5 & 53, \end{matrix}$

definition very unsatisfactory.

The Observatory, Dun Echt.

Aberdeen, 1877, Dec. 26.

On a Cyclonic Arrangement of the Solar Granules.

By W. Huggins, D.C.L., LL.D., F.R.S.

The splendid solar photographs which are taken by Dr. Janssen at Meudon, one of which he has kindly presented to this Society, shows, among other important features, a frequent

FIG. 1.



FIG. 2.



tendency of the granules to arrange themselves in a spiral form, accompanied by more or less loss of distinctness of outline of the individual granules, an appearance which suggests great

cyclonic disturbances of the part of the solar atmosphere in which these bodies lie. It may be of interest, perhaps, to put on record a similar appearance which was observed in 1866.

The Notes made at the time read thus:—

“Saw distinctly the granules. A spiral band of closely associated granules, ending in one of larger size (fig. 1). In one area near the centre of the Sun’s disk, the granules appeared more elongated than usual (fig. 2), rather sparsely scattered, and the larger diameters very nearly in the same direction. In neighbouring areas the granules smaller and less elongated. Amongst these no general direction was observed.”

On the Probable Errors of Transits of the First and Second Limbs of the Sun, observed by the Chronographic Method. By A. W. Downing, B.A. (Dublin).

It has always appeared to me, and I believe the impression prevails amongst most persons who are in the habit of constantly observing transits of the Sun, that the transit of the first limb is a more difficult observation than that of the second. This difficulty is occasioned, no doubt, by the fact, that in the case of the first limb the wires of the transit instrument are rendered faint by the glare of the advancing limb, whilst in the case of the second limb they appear as dark lines projected on the bright disk of the Sun as a background, and can be watched with ease during the transit of the limb. It occurred to me then, bearing this circumstance in mind, that it would be a matter of some interest to ascertain if the probable errors of transits of the Sun’s first limb were sensibly greater than those of the second, and I accordingly entered upon the investigation, the results of which I have now the honour of laying before the Society. The materials used in the computation were the observations of the Sun made at Greenwich with the transit-circle from 1874, July, to the end of 1875, 100 complete transits having been observed during that time, and the mode of reduction being as follows: The time of transit over each separate wire has been reduced to the central wire by applying to the recorded time the adopted equatoreal interval between the wire and the central wire, corrected for Sun’s declination and increase of right ascension in the interval, and thus for each complete transit we have nine “central wires.” I then take the mean of these, and find the difference of each from the mean. This being done for each complete observation, we have a number of “apparent errors,” from which the probable error is found by the usual formula.

The following Table exhibits the resulting probable errors found by this process, and I have added, for comparison, the results for the transit of a star, given by Mr. Dunkin in the *Monthly Notices*, vol. xxiv., p. 154:—

Object.	Probable Error of a Transit over one wire. s	Probable Error of a complete Transit. s
☉ 1 L.	$\pm 0^{\circ}072$	$\pm 0^{\circ}024$
☉ 2 L.	$\pm 0^{\circ}063$	$\pm 0^{\circ}021$
☉ Centre	$\pm 0^{\circ}048$	$\pm 0^{\circ}016$
A Star	$\pm 0^{\circ}051$	$\pm 0^{\circ}017$

It appears from this Table that the probable errors of transit of the first limb is slightly greater than that of the second; both being, as might be expected, somewhat greater than that of a star.

It must be remarked that the numbers given above comprehend not only the errors of observation, but also those of the adopted equatoreal intervals of the wires and the errors arising from the operation of translating the punctures on the chronograph-barrels into figures; but these may safely be assumed to affect equally the observations of the first and second limbs, and so do not influence the relative magnitudes of the resulting probable errors.

I have, next treated separately the observations made by each person, and so found five results, one for each of the four regular observers, and one for a miscellaneous group of junior observers, composed chiefly of supernumerary computers. The observations are perhaps not sufficiently numerous to determine with accuracy the probable errors of each observer's transits, but they enable us to see the general tendency. The results are given in the following Table and are compared, as far as possible, with the corresponding values for a star, as in the first Table:

Object.	Probable Error of a Transit over one Wire.				
	Ellis. s	Lynn. s	Criswick. s	Downing. s	Junior Observers. s
☉ 1 L.	$\pm 0^{\circ}059$	$\pm 0^{\circ}059$	$\pm 0^{\circ}051$	$\pm 0^{\circ}061$	$\pm 0^{\circ}086$
☉ 2 L.	$\pm 0^{\circ}050$	$\pm 0^{\circ}058$	$\pm 0^{\circ}058$	$\pm 0^{\circ}061$	$\pm 0^{\circ}068$
☉ Centre	$\pm 0^{\circ}039$	$\pm 0^{\circ}041$	$\pm 0^{\circ}039$	$\pm 0^{\circ}043$	$\pm 0^{\circ}055$
A Star	$\pm 0^{\circ}053$	$\pm 0^{\circ}052$	$\pm 0^{\circ}048$	...	...

It is thus seen—confirming the opinion that the transit of the first limb is the more difficult observation—that the larger probable error found in the general result for the transit of the Sun's first limb is mainly due to the observations made by the junior observers, the more experienced assistants observing both limbs with nearly equal accuracy; in fact, the general result for the four senior observers is practically identical for both limbs, viz. for the first $\pm 0^{\circ}057$, and for the second $\pm 0^{\circ}058$, for a transit over one wire.

We may conclude, then, that for observers of experience the mean results of transits of the Sun observed by the chronographic method will be equally accurate for both limbs; but that for persons who are only occasionally employed in observing transits there will be a tendency to observe the first limb with less

accuracy than the second, the transit of the first limb being the more difficult phenomenon to observe, and requiring care and practice for its accurate observation. The only account of a previous investigation of this nature that I know of is to be found in the *Proceedings of the Royal Irish Academy*, vol. vii. p. 371, where Dr. Robinson states that, from his observations made at Armagh in 1830, he found for the probable error of transit of Sun's first limb $\pm 0^{\circ}116$ and for that of second limb $\pm 0^{\circ}087$, whilst stars gave the result $\pm 0^{\circ}097$ (these observations, of course, were made by the "eye and ear" method); but I can discover no further details of this computation.

As an *addendum* to this paper, though not immediately connected with the subject of it, I may mention that I find that Argelander states (*Vierteljahrsschrift der Astron. Gesellschaft*, 1872, p. 16) that a transit over the first wire of a set is not as accurate as that over succeeding wires; and as I had the materials at hand, I thought I might as well test the statement in the case of the transits of the Sun observed at Greenwich. I have accordingly computed the probable errors of transits of the Sun's limb over wire i. and over wire ix. of the system of nine wires used for the observation of transits by the galvanic method, with the following result:—

	Probable Error of a Transit	
	over Wire i.	over Wire ix.
	s	s
☉ 1 L.	$\pm 0^{\circ}075$	$\pm 0^{\circ}073$
☉ 2 L.	$\pm 0^{\circ}071$	$\pm 0^{\circ}070$,

thus showing a slightly smaller probable error for wire ix.; but I know not whether it is decided enough to confirm Argelander's opinion, or ought to be considered as merely accidental.

On the Evidence of Periodic Variability of the Nebula H. II. 278,
1860.0, $\alpha = 2^{\text{h}} 23^{\text{m}} 25^{\text{s}}$, $\delta = -1^{\circ} 43' 0''$. By Prof. Winnecke.

Astronomers are unanimous, with regard to the variability of nebulae, only about the variability of *one* Nebula, the very interesting object discovered by Mr. Hind, 1852, October 11, in $\alpha = 4^{\text{h}} 13^{\text{m}} 48^{\text{s}}$, $\delta = +19^{\circ} 11' 2''$ for 1860, which has been easily observed with the annular micrometer of a $4\frac{1}{2}$ -inch refractor by D'Arrest in the years 1855 and 1856. This Nebula has now completely vanished; it is not visible at present in our most powerful telescopes.

During the construction of the University Observatory at Strasburg, and in expectation of the 20-inch refractor* by Merz, mounted by Repsold, which is intended to be used for a thorough

* Merz has finished two very good object-glasses of 20-inch free aperture; one of them has been purchased, after severe trial by myself, for Strasburg in September last.

revision of the nebulae, I commenced working, more than two years ago, in this field of research with an orbit-sweeper of $6\frac{1}{2}$ -inch aperture* (object-glass by Reinfelder and Hertel of Munich, mounting and micrometer by Repsold of Hamburg), and I have now micrometrical measures of more than 300 nebulae, with the stars in their vicinity, generally on more than one night.

In comparing the former measures and descriptions with my own, strange differences are not seldom noticed; one of the most curious instances is presented by the Nebula H. II. 278 (1860.0, $\alpha = 2^h 23^m 25^s$, $\delta = -1^\circ 43' 0''$). I find the following observations:—

H.	1875	Jan. 6	pB. S.E.; 1 obs.
h.	1827	Nov. 24	pB. S.E.; bM.
h.	1827	Dec. 20	pB. S.E.; p5bM.
R.	1856	(Nov. 29?)	Nothing particular.
D'Arrest	1856	Jan. 2	Nicht klein, doch sehr schwach. (Refractor by Merz, $4\frac{1}{3}$ -in. ap.)
Schönfeld	1861	Dec. 2	Ich habe, 1861, Dec. 2, bei Luft I mich nicht sicher von seiner Existenz überzeugen können, sondern nur notirt: <i>Vielleicht gesehen</i> . Die Sichtbarkeit ist allerdings vielleicht durch den Stern 75 <i>Ceti</i> ($5^m.6$), der nördlich folgt, etwas erschwert. (Refractor by Steinheil, $6\frac{1}{2}$ -in. ap.)
D'Arrest	1863	Nov. 3	Non egregie serenum. Subclara, minuta; manifesto haud "majuscula" ut in observatis meis Lipsiensibus annotatum reperio. Nucleum habet fixae 13^m haud dissimilem. Ipsa nebula oblonga, et quidem notabiliter. * 13 ord. proxime seq. ad austrum.—Turbulenter omnia: densa nubes eripuit nebulam. (Refractor by Merz, $11\frac{1}{2}$ -in. ap.)
D'Arrest	1863	Nov. 7	Sudum conspicua, ellipsis satis stricta. Nucleum monstrat fixae $11^m.12$ similem. 12 ord. seq. $5.75^s 2'$ vel $3'$ ad meridiem.
Schönfeld	1864	Feb. 2	Luft 1–2. Mässig schwach. Scheint nicht rund. 5bM. Kernlicht = $12^m.2$, aber nicht sternartig. 12 ^m seq. A dist. $2\frac{1}{4}'$. (Steinheil, $6\frac{1}{2}$ -in. ap.)
D'Arrest	1864	Nov. 6	Apprime serenum. Clara, longula; nucleus = * 13 mag. * 12^m mag. seq. $5^s.6$ nonnisi $1\frac{3}{4}'$ ad meridiem.
Vogel	1865	Nov. 13	Bei äusserst durchsichtiger Luft vergeblich gesucht, desgleichen Nov. 20. (Steinheil, $8\frac{1}{2}$ -in. ap.)
Schönfeld	1868	Jan. 16	Luft 2–3, $6''$, rund, Kern fast sternartig $11^m.12$, 11^m seq. A dist. $3'$.

* For a notice on the beginning of these observations and a description of the orbit-sweeper and its micrometers, see *Vierteljahrsschrift der Astron. Gesellschaft*, Band x. pp. 297–305.

Winnecke	1877	Jan. 6	Gut II Klasse, $1\frac{1}{2}'$ gross mit ... I. Mit 170 facher Vergr. $30''$ l., $20''$ br., verlängert in $p = 7^{\circ}2$.
Winnecke	1877	Jan. 10	Ein recht heller Nebel, II Klasse, $30''$ l., $20''$ br., zur Mitte stark, fast fixsternartig verdichtet.
Winnecke	1877	Sept. 14	Ogleich der Himmel sich wieder ziemlich stark mit Cirrus bezogen hat, sehr gut sichtbar, $1'$ lang, gedehnt in $p = 20^{\circ}$; scheint einen fixsternartigen Kern = $12^m.13$ zu haben.

The descriptions of the Nebula by the Herschels agree entirely; nor has Lord Rosse noticed anything particular. But in December 1861 the Nebula, which had been observed five years before by D'Arrest with a much smaller refractor, was *invisible* to Schönfeld with the splendid Mannheim refractor. In the years 1863 and 1864 it is observed both by Schönfeld and D'Arrest; 1865, November 13 and November 20, it is *again invisible* to the sharp eyes of Dr. Vogel with the Leipzig refractor. It may be noticed that an error in pointing the telescope on these days is impossible, for Dr. Vogel has observed other nebulae near it on November 13 and 20. Schönfeld observed the Nebula again in the year 1868 without difficulty, and in the present year it is a *very easy* object for $6\frac{1}{2}$ -inch aperture.

It seems, therefore, probable that the quantity of light from this object reaching our eyes is very different in different years; a most important conclusion, as it is the first instance of a very great evidence for *periodic variability* of a Nebula.

Strasburg, 1877, Dec. 11.

On a Cause for the Appearance of Bright Lines in the Spectra of Irresolvable Star Clusters. By E. J. Stone, M.A. Camb., Her Majesty's Astronomer, Cape of Good Hope.

In the Supplementary Number of the *Notices*, vol. xxxvii., there appears a short paper by Mr. Proctor "On the Arc of Light seen round *Venus* in transit." To this paper are appended some remarks which are, I presume, intended to be a criticism of my paper which appeared in the *Proceedings of the Royal Society*, No. 180, 1877. It appears to me, however, that Mr. Proctor has missed the point of my paper; and as I should prefer that my meaning be understood, I shall give Mr. Proctor's remarks and a statement of my views, extracted from the *Proceedings of the Royal Society*, for comparison with each other.

But before proceeding to this point I may perhaps be allowed to mention that Mr. Proctor's paper "On the Arc of Light seen round *Venus* in transit" is a particular case of the problem investigated by Sir G. B. Airy in the *Monthly Notices*, No. 1, vol. xxiv., 1863. Sir G. B. Airy proves that, if a part of the Sun's disk is viewed through a refracting medium which produces

an effect analogous to that of a convex lens, that part of the disk will not be of greater apparent brightness than the rest; or, "that no refraction can cause a change in the apparent brightness of the surface visible." As Prof. Airy's investigation is very complete and covers the ground of Mr. Proctor's paper, I presume that Mr. Proctor must have been unacquainted with the investigation at the time his paper was written.

The following are Mr. Proctor's remarks upon my paper:—

"It is asserted that an irresolvable stellar nebula would diminish in apparent brightness (so far as the stellar part of its light is concerned) with increase of distance. No reasoning is given in support of this assertion, and I can see no reason for withdrawing the reasoning by which, in a paper 'On the Resolvability of Nebulæ regarded as a test of distance,' I show (incidentally) that a nebula, so long as it remained irresolvable, would be of constant intrinsic brightness whatever its distance."

My statement of my views is as follows:—

"The Sun is known to be surrounded by a gaseous envelope of very considerable extent. Similar envelopes must surround the stars generally. Conceive a close stellar cluster. Each star, if isolated, would be surrounded by its own gaseous envelope. These gaseous envelopes might, in the case of a cluster, form over the whole or a part of the cluster a continuous mass of gas. So long as such a cluster was within a certain distance from us, the light from the stellar masses would predominate over that of the gaseous envelopes. The spectrum would therefore be an ordinary stellar spectrum. Suppose such a cluster to be removed farther and farther from us. The light from each star would be diminished in the proportion of the inverse square of the distance; but such would not be the case with the light from the enveloping surface formed by the gaseous envelopes. The light from this envelope received on a slit in the focus of an object-glass would be sensibly constant, because the contributing area would be increased in the same proportion that the light from each part is diminished. The result would be, that at some definite distance, and all greater distances, the preponderating light received from such a cluster would be derived from the gaseous envelopes, and not from the isolated stellar masses. The spectrum of the cluster would therefore become a linear one, like that from the gaseous surroundings of our own Sun. The linear spectrum might, of course, under certain circumstances, be seen mixed up with a feeble continuous spectrum from the light of the stars themselves.

"It should be noticed that, in this view of the subject, the linear spectrum can only generally appear when the resolvability of the cluster is at least injuriously affected by the light of the gaseous envelopes becoming sensibly proportional to that from the stellar masses, and that in the great majority of such cases it would only be in the light from the irresolvable portions of the cluster that bright lines could be seen in the spectrum."

When the paper was read, it appears that Professor Stokes objected that, although the contributing area of the gas would be increased in the way indicated, yet other stellar masses would at the same time be crowded into the line of sight, and that it need not therefore follow that the spectrum should become linear with increase of distance.

So long as the gaseous envelopes are considered to be isolated round each individual star, and to be of comparatively speaking small dimensions, the objection is a valid one; but the clusters which I had under contemplation were physical clusters, and this isolation of the gaseous surroundings would be, under suitable conditions, destroyed by the relative gravitation and heating effects of the several members of the system. It is to such a cluster only that my reasoning applies; and I think such will be seen to be the case if my paper is carefully read.

I need hardly say that, after I heard of Professor Stokes' objections, I carefully reconsidered the theory; but after the best consideration I can give the subject, I believe my conclusion is sound, and there the matter must rest for the present so far as I am concerned.

With respect to Mr. Proctor's own views: If he thinks that he has proved that, because a stellar cluster is irresolvable with a telescope of small aperture and small power, it must follow that "it would be of constant intrinsic brightness at all greater distances," I think there would be no great difficulty in proving that he is mistaken. I have introduced the words "small aperture" and "small power" as presenting the case in an exaggerated form, and merely to indicate the point upon which I think his theory fails.

I must also, as bearing on the same point, call attention to a very general statement of Mr. Proctor's, that "he has shown that by no optical contrivance can apparent brightness be increased, though quantity of light can of course be greatly increased." This statement certainly requires qualification; for, if it were true, why could not the satellites of *Mars* be as easily seen with a 4-inch as with the 26-inch of the Washington Observatory? I know, of course, what Mr. Proctor should, and probably does, mean; and I should not have called attention to this point had it not been the fact that the principal cases of exception are those which have been under consideration in the present paper.

Note on Specular Reflexion from Venus.

By W. H. M. Christie, Esq.

In the *Monthly Notices* for December 1876, I gave some observations on the gradation of light on the disk of *Venus*, made with the object of testing Mr. Brett's hypothesis of specular reflexion

modified by atmospheric diffusion. The position of the brightest point of the disk, determined with all possible care, agreed with that assigned by this hypothesis, whilst it was incompatible with any theory of reflexion from an unpolished surface. The disk was then somewhat gibbous, and the whole question turned on the estimate of position, about which, however, I feel pretty confident. I have now made some observations of *Venus* in the crescent form with the Greenwich Equatoreal, the result being entirely to confirm Mr. Brett's original statement, that the brightest part of the crescent is a little within the limb. On December 31, 1877, I found, on reducing the intensity of light by means of the polarising eye-piece previously referred to, that the breadth of the crescent diminished from about 0.8 of the angular radius of the disk to 0.2, fading off on both sides as well as towards the cusps; and in the *middle* of the breadth of this narrow strip I was able to detect a very fine line, the length of which I estimated at about one-fourth of the radius. This observation was made in the evening between 5^h and 6^h. Allowing for the loss of a small portion on the side of the limb (when the light was reduced to one 3000th part), of which I satisfied myself by repeated comparisons, I estimated this brightest portion of the disk to be 0.2 of the radius within the limb. The calculated position on the hypothesis of specular reflexion, as afterwards found, would be 0.22 of the radius from the limb. On 1878, January 9 and 10, I had an opportunity of examining *Venus* in the daytime, and was fortunate in securing Capt. Tupman's co-operation on these occasions. As seen with an ordinary eye-piece (power 500) *Venus* appeared as a crescent, having a breadth equal to about two-thirds of the radius, with a bright arc near the limb. This arc shaded off perceptibly towards the cusps. On applying the solar eye-piece, which reduces the light (by virtue of the three reflexions from glass) to one 3000th part, both Capt. Tupman and I satisfied ourselves that there was a bright elongated patch distinctly within the limb. In the centre of this I caught glimpses of a small point of light, the distance of which from the *apparent* limb I estimated as rather more than 0.1 of the radius. The position of the point at which specular reflexion would take place, I afterwards found by calculation to be 0.18 of the radius within the limb.

Although the estimates of position of the bright point are, from the nature of the case, rough, the agreement with the specular reflexion hypothesis is very striking; and these observations would establish the fact that there is a gradation of light on the disk of *Venus*, which can only be explained on that theory. On the supposition of reflexion from a rough or unpolished surface, there would, according to Mr. Neison, be a point of maximum illumination, but it would fall on the farther side of the limb, and as the limb itself, at the time of the second observation (January 9), was distant 20° of longitude from this point, the increase of brightness would be very marked right up to the limb. Now the actual circumstances are very different. There

is a portion of the disk near the limb which at a first glance appears to be uniformly brighter than the part near the terminator. The breadth of this bright arc was, on January 9, about one-fourth of the radius, so that at its inner edge the Sun's zenith distance must have been 60° , whilst at the limb it would be 20° . Thus, if we had to do with reflexion from a rough surface, there ought to be a decided gradation up to the very limb, the brightness at that part being, on this supposition, twice as great as at the inner edge of the bright arc. This is emphatically not the case.

The bright patch here referred to was described by Capt. Noble in the *Monthly Notices*, June 1876, when *Venus* was under similar conditions of illumination, and his failure to detect any gradation whatever towards the limb appears conclusive against the theory of reflexion from a rough surface. Had he looked a little more closely, he would, perhaps, have detected in the middle of the bright patch a small bright point, as I did. At any rate, the existence of a patch of sensible area, and "without the slightest sign, trace, or indication of one part being in the smallest degree brighter than another," seems utterly incompatible with any theory whatever. But on the hypothesis of specular reflexion modified by diffusion in a dense atmosphere, the point of maximum brightness would be in the middle of this patch, and the gradation on either side, being comparatively slight in the neighbourhood of the maximum, might easily be overlooked by a hasty observer. But I believe that, by patient gazing under favourable circumstances, the actual image of the Sun may be seen as a minute speck. A high power, however, must be used, as the speck would hardly exceed $0''.1$ in diameter. I have used throughout a power of 500. The reduction of the light by a dark glass or other means is important, as it removes any disturbing effect of irradiation and enables the eye to appreciate more readily small variations of brightness. It may be well to add, that the position of plane of polarisation in the eye-piece relatively to the line of cusps appeared to have no appreciable effect on the gradation of light. This would be consistent with the theory of reflexion from a metallic surface; but the observation is so delicate, that I would not lay much stress on the circumstance that no trace of polarisation was noticed.

1878, January 10.

P.S.—On 1878, January 11, after the above was written, Mr. Maunder examined *Venus* both with the polarising eye-piece and a dark wedge, and noted a bright line or elongated patch, about $7''$ or $8''$ in length and $2''$ in breadth, the middle of which was from $2\frac{1}{2}''$ to $3''$ within the limb, i.e. about 0.15 of the radius. As the brightness was still further reduced the ends of the bright line disappeared, and the last phase was a tiny faint blur. These observations were quite independent, and Mr. Maunder had, in fact, no knowledge of my results.

*Suspected Repetition, or Second Outbursts from Radiant Points;
and on the Long Duration of Meteor Showers.* By W. F.
Denning, Esq.

The radiant-points which I have reduced—chiefly from my observations of the paths of 2,690 shooting stars between April 1876 and December 1877, and from the Catalogue of the Italian Meteoric Association (1872), which I have recently been comparing together and with the positions obtained by other observers—appear to prove that many showers have very long duration. In some well-marked cases the period extends over four months at least, which is much greater than that commonly assigned to them; in fact, I believe that, of the showers previously known, not more than three last longer than eight or nine weeks, and certainly none longer than twelve weeks. My own observations and reductions would, however, considerably extend the duration generally, and there appears to be a tendency in many radiants to repeat themselves (or give secondary outbursts), at an interval of about three months after the first display. There is no absolute cessation of action during the intervening lull, though meteors come very sparingly, and seldom in sufficient number to constitute a “shower,” on one or two nights. It is true that there are now so many known radiant-points differing in dates, positions, intensities, &c. (and these only approximately ascertained in numerous instances), that it is but fair to expect many accidental coincidences in position such as I have been referring to, and that they are merely different showers coming later from same places. This seems the natural explanation, and it is rendered more probable by the fact that meteor showers make their appearance, as a rule, round about the Earth’s apex, and are rarely seen active or well defined far away from it. In fact, these systems are generally confined to a minor part of the heavens, and thus it would be singular indeed if, with so many different showers, we had no instances in which the radiant positions were found to overlap or overlies. There is also this to be said, that absolute accuracy is never attained in this branch of observation, and that a radiant of a feeble system can seldom be relied upon, except in special cases, to within five or seven degrees. Moreover, the idea of these repeating radiants (assuming them to belong to the same parent system), is opposed to the relation of comets and meteors in the simple form we understand it, and cannot easily be accounted for. It seems to me, however, that, notwithstanding all this, the large number of the agreements and their exact nature are strongly suggestive of some connection, and require a different explanation to that indicated above. I have no longer any hesitation in mentioning it as a point deserving special attention from workers in this department. In the present very imperfect state of our knowledge it would be wrong to hastily condemn any observed facts, however improbable and incomprehensible they may at first sight appear.

The following are a few cases of these radiants which I have selected as samples of many others that might be adduced. The positions are from my own observations or the Italian ones, and a few are included that I reduced from the late Dr. Heis's recently published work, *Resultate der in den 43 Jahren 1833-75, angestellten Sternschnuppen Beobachtungen* (Munster, 1877), and Zezioli's Catalogue of 8,623 meteors, 1867-70, which had, however, already been partially reduced, the former by Heis himself, and the latter by Schiaparelli. The various radiants determined from these sources are incorporated, and an average taken, but only in cases when they agreed very closely, and unmis- takably belonged to identical systems:—

II. Second Display.

I. First Display.

Dates.	No. of Positions.	No. of Meteors.	Radiant-point. R.A. Decl. ° °	Radiant-point. R.A. Decl. ° °	No. of Positions.	Dates.
Aug. 6-12	2	61	97 + 71	101 + 73	3	Oct. 2—Dec. 2
Aug. 6—Sept.	4	63	46 + 73	43 + 73	4	Oct. 28—Nov.
Aug. 6-12	1	21	76 + 33	77 + 31	2	Oct. 2—Nov. 13
Aug. 6-16	3	61	70 + 64	69 + 66	5	Nov.—Dec.
July—Sept.	5	76	62 + 48	62 + 48	5	Oct.—Nov. 13
Aug.—Sept.	5	82	61 + 37	61 + 36	4	Oct.—Dec.
Aug.—Sept.	5	41	44 + 24	45 + 25	4	Oct.—Nov.
Oct. 2—Dec.	4	53	131 + 20	132 + 20	3	Feb.—April
Nov. 22—Dec. 13	2	47	81 + 23	83 + 22	2	Feb.—Mar. 12
February	1	10	132 + 67	134 + 67	2	April—May 15

* New showers requiring confirmation.

I believe the 10 pairs of radiant in this Table are accurately placed; they are supported by a large number of meteors and (with four exceptions) agree with showers seen by former observers. Many other such agreements as are here presented will be found by consulting the radiant-lists of Greg and Herschel, Heis, Schmidt, Schiaparelli and Zezioli, and Tupman. The following are a few additional ones from my own observations:—

Mar. 18—May 17	⁰ 286— ⁰ 13	⁰ 284— ⁰ 11	July 6—Aug. 16.
Sept.—Nov.	130+47	130+44	Jan. 4—20.
April 21—May	280+58	284+57	July 16—Aug. 16.
Dec. 10—Jan. 20	221+41	220+40	Mar. 14—April.
Aug. 3—16.	31+18	30+16	Oct. 28—Nov. 13.
Nov.—Dec.	173+9	175+10	March.
Nov.—Dec.	182+36	181+34	Feb. 20.
Oct. 2	225+52	221+52	Dec. 12—Jan. 20.
April 16—19	267+21	267+20	Aug. 5—Sept. 7.
May 15	304+7	306+6	Aug. 3—16.

The number might be greatly extended. Thus, there are apparently recurring showers in the months of July and October from $30^{\circ}+36^{\circ}$, $47^{\circ}+45^{\circ}$, $9^{\circ}+34^{\circ}$, $80^{\circ}+56^{\circ}$, $6^{\circ}+52^{\circ}$, and in October and January from $105^{\circ}+50^{\circ}$, $104^{\circ}+13^{\circ}$, $153^{\circ}+42^{\circ}$, $120^{\circ}+15^{\circ}$, $136^{\circ}+78^{\circ}$, &c.; and it appears certain that a radiant-point agreeing with that of the August *Perseids* is continued from that month to December! An average of 14 positions gives $45^{\circ}+57^{\circ}$ as the centre of it. Now Mr. Greg's general central radiant for the August *Perseids* is at $44^{\circ}+56^{\circ}$, which accords almost precisely with the preceding one. The November *Perseids* Mr. Greg places at $46^{\circ}+55^{\circ}$, and the position was well confirmed by Heis at $44^{\circ}+56^{\circ}$ from 48 meteors seen during the first half of the month. Thus it is evident that the August and November *Perseids* have precisely the same radiant-point. Not only Heis, but Greg and Herschel, and Schiaparelli and Zezioli saw the shower well defined in November. From my own observations and those made in Italy, the radiant also appears tolerably active during the first half of September, and Captain Tupman saw it accurately at $42^{\circ}+55^{\circ}$ on the morning of September 5, 1870. These later meteors from *Perseus*, seldom have streaks like those of about August 10, and it is very probable that they belong to quite a distinct system. There is no doubt, however, that after the maximum on August 10 there is still a lingering shower from the same centre, and that it reaches its greatest intensity about three months after the first display, which confirms the periods observed in revivals of other showers.

It is a peculiar thing to say that a person may see, by looking towards the eastern sky (at the constellations of *Perseus*, *Auriga*, and *Camelopardalus*) in the evenings of November, many of the same radiant points that were visible in the early mornings of August,

or that the radiants observable in the mornings of November come into play again in the evenings of February, &c.; but that seems a probable conclusion from numerous observations and reductions. Fortunately the question may soon be settled without waiting for further observations. The Catalogues before mentioned contain hosts of well-observed meteor paths from which it will be possible to confirm or disprove these reappearing meteor systems, though the work will entail much labour, and there will be some difficulty in getting at the radiant-points accurately. I would suggest that, if it is undertaken, the meteors for August 1-15 be first examined, and those presumably belonging to radiants *eastwards of Perseus* (say between 50° - 110° R.A. and 20° - 80° North Decl.) selected. This may be done by a glance at the places of beginning and ending, and in this way the mass of ordinary *Perseids* may be avoided. A series of similar star charts must be used in the projections, and the radiant-points determined from each, then the positions of the nearly accordant showers averaged. Following the same plan for the first half of November, the two results would admit of fair comparison. There would be some showers in November not seen in August and *vice versâ*, but in many instances, I believe, the positions will be identical, or so nearly accordant as errors of observation and reduction will allow. I shall look forward with much satisfaction to the confirmation of a new August shower in *Camelopardalus* at $97^{\circ} + 71^{\circ}$, which I found splendidly defined from two independent sources, and it is difficult to understand how Heis missed this radiant (though he saw many of its meteors) during the 43 years that he gave great attention to the meteors of August.

Postscript.

Since writing this paper I have partially reinvestigated the positions on which I chiefly rely as proving these repeating meteor showers, and have been projecting the paths of many hundreds of shooting stars recorded during the first half of January, with the view to ascertain whether a number of radiants I observed in the mornings of last October recurred again, as they should according to my previous observations. The result of the examination was very satisfactory in each case, and apparently proved in the most palpable and conclusive manner the suspicion I have ventured to mention in this paper, namely, that there is more than one outburst from radiant-points, and two maxima at intervals of about three months, little more or less. I do not mean to say that they can possibly represent the same parent systems, and that the two displays are simply manifestations of one stream, because that is incredible and unaccountable from existing theories. The subject is very wide and difficult, requiring yet much further assiduous work before we may hope to unravel the vast and complicated network of meteor streams which the Earth annually encounters.

Remarks on Mr. Denning's Paper. By Captain G. L. Tupman.

The radiant-point of a meteoric shower very seldom indicates the true direction of motion. When the orbital motion of the Earth is transverse to the true direction of motion of the stream, the radiant is displaced to the extent of 35° towards the apex of the Earth's way (a point in the ecliptic approximately 90° of longitude behind the Sun), on the supposition of the greatest velocity that can reasonably be assigned to meteors—that proper to parabolic motion—which for the present it will be sufficient to consider.

All the meteor streams we are certainly cognisant of at the present time can be accounted for by supposing them to belong to the solar system in the same manner as do comets. In the case of certain streams, as the *Leonids* of November 13, and the *Perseids* of August 10–11, the identity of their orbits with those of known comets is too certainly established to need comment, and these may be taken as typical streams belonging to the solar system.

The spreading out of the individuals in directions perpendicular to the general direction of motion necessarily involves a different orbit for each individual, and in proportion as the spreading out becomes more considerable, there will be less similarity of orbit.

The term *stream* or *shower* should properly be applied to the *ensemble* of a number of particles moving approximately in the same orbit in nearly the same periodic time. It is evident that we can have no knowledge whatever except of those streams that actually intersect the orbit of the Earth. There is a special case in which the Earth can remain in a stream for several weeks with a nearly fixed radiant. The orbit must nearly coincide with the plane of the ecliptic, the perihelion distance of the central portion be a little less than unity, and the motion be direct. The position of the radiant would be 90° before the Sun at the middle time.

If the perihelion distance be supposed to diminish, say to $\cdot 8$, the Earth would pass through the central part of the stream twice at an interval of about $3\frac{1}{2}$ months. At the first passage the radiant would be about 125° before the Sun, at the second about 30° , and therefore only to be observed shortly before sunrise. In all other cases the reappearance of a radiant after an interval indicates a distinct shower.

The results obtained by Mr. Denning after so much labour are probably to be attributed to the method employed in obtaining the radiants. The method was begun by Heis, and used by Greg, Herschel, Schiaparelli, and others, and was indeed the only method available with few observations. The observations of many nights were combined to obtain the apparent radiation, and the duration was supposed to begin and end with the first and last day so employed.

It is easy to obtain a radiant in any part of the sky, especially near the apex of the Earth's way, by collecting, night after night, meteors whose tracks, carried backwards, pass near the point in question.

I understand that Mr. Denning's coincidences in no degree depend upon previous knowledge; each is an entirely independent deduction, and therefore much more striking and singular.

We are now, probably, in possession of sufficient observations to enable the radiants proper to each day of the year to be deduced separately, and the number of determinations from multiple observations of solitary bright meteors is steadily increasing. These last are the best of all.

At present we are not obliged to resort to the idea of immense streams of meteors existing in space, and brought into collision with us by the motion of the Sun. The radiant-point of such a stream, moreover, would not be stationary many weeks.

Until the durations of fixed radiants be established in a more certain manner than those in the paper before us, we need not endeavour to explain them.

On a Small Term of Long Period in the Mean Motion of the Moon.

By E. Neison, Esq.

The mean motion of the Moon contains a term of long period, with an argument of the form

$$\{[a - 29n'' + 26n'] t + \epsilon - 29\epsilon'' + 26\epsilon' - E - A_0\},$$

where $(at + \epsilon - A_0)$ denotes the mean anomaly of the Moon, and ' and '' quantities relating to *Venus* and the Earth respectively. This term has a period of 130 years, and it appeared probable that its coefficient would be sensible, for it was evident that the terms depending on the eccentricities would not suffer the internal mutual destruction which occurs in the case of the analogous *Venus* term discovered by Hansen. The value of the term was calculated, but the relative positions of the perihelia of the Earth and *Venus* render its coefficient only one-fifth of that it would have possessed had it arisen from the planet *Mars*.

It is, however, very desirable that, when the value of such a term has been determined, it should be placed on record in sufficient detail to save the useless labour of a repetition of the work by subsequent investigators in the same field. Moreover, small as the term is, it might well be included in any future lunar tables, for it has a comparatively short period.

Neglecting terms depending on the Moon's longitude and latitude, then the disturbing function may be written (Delaunay, *Conn. des Temps*, Add. p. 8, 1862)

$$R^{(1)} = m' r^2 \left\{ \frac{1}{4\Delta^3} - \frac{3}{2} \cdot \frac{1}{\Delta^3} \cdot \eta_1^2 \sin(v' - B') \cdot r' [r'' \sin(v'' - B') + 2r' \sin(v' - B')] \right\},$$

the notation being slightly altered to bring it into accordance with my other result. Suppose the terms of the third order of the eccentricities and inclinations to be neglected, except as contained in the function Δ , and replace Δ^{-s} by its general term,

$$\Delta^{-s} = (a')^{-s} \cdot [B_{\frac{s}{2}}^s]_{i+k}^i \cdot \cos \{[in'' - (i+k)n']t + E\};$$

then the disturbing function becomes

$$\begin{aligned} R = & m' (a')^{-3} r^2 \left\{ \frac{1}{4} [B_{\frac{3}{2}}]_{i+k}^i \cos \{ [in'' - (i+k) n'] t + E \} \right. \\ & \left. + \frac{3}{8} \eta_1^2 \left(\alpha^2 [B_{\frac{3}{2}}]_{i+k \pm 2}^i + 2\alpha [B_{\frac{5}{2}}]_{i+k-1}^{i+1} + 2\alpha [B_{\frac{3}{2}}]_{i+k+1}^{i-1} \right) \cos \{ [in'' - (i+k) n'] t + E \pm 2B_1 \} \right\} \end{aligned}$$

Giving i and k the values necessary to bring the indices of $[B_{\frac{g}{2}}]$ to the values 26 and 29, then

$$\begin{aligned} \mathbf{R} = m' (a')^{-3} r^2 \left\{ \frac{1}{4} [\mathbf{B}_{\frac{5}{2}}^{\frac{29}{26}}]_{26} \cos \{ [29n'' - 26n'] t + \mathbf{E} \} \right. \\ \left. + \frac{3}{8} \eta_1^2 \left(\alpha^2 [\mathbf{B}_{\frac{5}{2}}^{\frac{29}{28}}]_{28} + 2\alpha [\mathbf{B}_{\frac{5}{2}}^{\frac{28}{27}}]_{27} \right) \cos \{ [29n'' - 26n'] t + \mathbf{E} - 2\mathbf{B}_1 \} \right\} \end{aligned}$$

Terms of the fourth order have been neglected, and α represents, as usual, the ratio $\frac{\alpha''}{\alpha'}$.

If (R) denotes the term in brackets, R may be written

$$R = m' (a')^{-3} r^2 \cdot (R) \cdot \cos \{ [29n'' - 26n'] t + E_1 \}.$$

Replacing r^2 by its value, and writing $m' \frac{a^3}{a'^3} = \sigma m^2$, and

$$\delta n = -3 \iint a n^2 \left(\frac{dR}{d\epsilon} \right) dt^2 = \sigma m^2 \cdot \left(\frac{n}{p} \right)^2 \cdot 3e \cdot (R) \sin \{ [\alpha - 29n'' + 26n'] t + \epsilon - E_1 \}.$$

The coefficient of t in the argument is $p = 10,171''$. Therefore the value of $\sigma m^2 \binom{n^2}{p^2} 3e$ is $1,378''$. The value of $[B_{\frac{1}{2}}]$ can be found in the usual manner, from either Delaunay or Pontecoulant. Substituting the proper values in the expression for δn , it becomes

$$\begin{aligned} \delta n = & +0''.005 \sin \{ [\alpha - 29n'' + 26n'] t + \epsilon - 29\epsilon'' + 26\epsilon' - A_0 + 3\varpi' \} \\ & - 0''.067 \sin \left\{ \begin{array}{lll} & '' & '' \\ & '' & '' \\ & '' & '' \end{array} \right. + 2\varpi' - \varpi'' \left. \vphantom{\sin} \right\} \\ & + 0''.201 \sin \left\{ \begin{array}{lll} & '' & '' \\ & '' & '' \\ & '' & '' \end{array} \right. + \varpi' - 2\varpi'' \left. \vphantom{\sin} \right\} \\ & - 0''.189 \sin \left\{ \begin{array}{lll} & '' & '' \\ & '' & '' \\ & '' & '' \end{array} \right. + 3\varpi'' \left. \vphantom{\sin} \right\} \\ & + 0''.049 \sin \left\{ \begin{array}{lll} & '' & '' \\ & '' & '' \\ & '' & '' \end{array} \right. + \varpi' + 2B' \left. \vphantom{\sin} \right\} \\ & - 0''.122 \sin \left\{ \begin{array}{lll} & '' & '' \\ & '' & '' \\ & '' & '' \end{array} \right. + \varpi'' + 2B' \left. \vphantom{\sin} \right\} \\ = & -0''.100 \sin \{ [\alpha - 29n'' + 26n'] t + \epsilon - 29\epsilon'' + 26\epsilon' - A_0 + 180^\circ .5 \}. \end{aligned}$$

By taking into consideration higher powers of the inclination, and the ordinary perturbations of the Moon, the coefficient of the term will be raised to nearly $-0''.150$.

On a Secular Term in the Mean Motion of the Moon.

By E. Neison, Esq.

The lunar co-ordinates contain a term of long period due to the action of *Mars*, and with the argument

$$\{[A - 15n'' + 28n'''] t + E\},$$

where At denotes the mean motion of the Moon's perigee, and $n't$ and $n'''t$ the mean motions of the Earth and *Mars*. This term has the very long period of 6,000 years, and is of the 14th order of the eccentricity of *Mars* and the Earth. The eccentricity of *Mars* is, however, so large that the coefficient of this term is comparatively considerable, despite its high order.

This term will introduce a similar term into the mean motion of the Moon, through the means of the solar perturbations; and this term will be multiplied by the square of the lunar perturbation arising from the action of the Sun, but divided by the cube of the small coefficient of the time. As

$$[A - 15n'' + 28n'''] = +197''.715,$$

the term will rise immensely, and will have, apparently, a considerable coefficient. Its calculation will be long and laborious, but, unless its terms destroy each other in some unexpected manner, it will repay the labour.

The present note, however, is directed to a single term with this general argument. The term

$$[A - 15n'' + 28n'''] - 9\varpi''' - 5\varpi''$$

has the coefficient of t reduced to zero from the motion of the perihelia of *Mars* and the Earth. It forms, therefore, a secular term in the mean motion of the Moon, and is, I believe, the first term of this kind due to the direct action of the planets which has been made known. It will obviously increase the secular acceleration by a small fraction, for it will appear in the mean motion as multiplying the square of the time.

Professor Newcombe is, I believe, the first who discovered that the mean motion of the Moon contains terms depending on the motion of the Moon's perigee, a result which I independently found two years back.

I know of at least two similar secular terms in the Moon's mean motion, but they will be much smaller, although they contain the Moon's mean motion in their arguments, for they depend on terms involving products of the fifth order of the lunar eccentricity and inclination.

The existence of these curious terms seemed of sufficient interest to merit their existence being announced.

On the Correction of the Elements of Ceres.

By William Godward, Esq.

The Ephemerides of Ceres in the *Nautical Almanacs* from 1857 to 1881 have been derived from Schubert's Elements, published in No. 69 of Gould's *Astronomical Journal* (Cambridge, U. S., 1854, April 3). These elements were founded upon 14 normal places 1832-1853, taking account of perturbations by *Jupiter* only, the differential coefficients of which were furnished by Professors Encke and Wolfers, from the calculations for the *Berliner Jahrbuch*. With the corrections given by the solution of equations of condition for the times of the 14 normals, Schubert computed the corrected elements for 1854, January 0, Washington mean time, which are the foundation of the *Nautical Almanac* calculations to 1881.

From these elements, with the addition of perturbations by *Venus*, the *Earth*, *Mars*, *Jupiter*, and *Saturn*, approximately perturbed elements have been computed for periods of 2,412 days at intervals of 36 days, and used for obtaining the variations of the elements more accurately. At the end of each period the elements were corrected by the sums of the variations, and the corrected elements used for the variations for the next period. The total change in the elements having been thus determined, it was expected that the *Nautical Almanac* places would fairly represent observation.

The following Table of Errors, which are taken as referring to the nearest date for which a direct computation for the *Nautical Almanac* has been made, is formed from the observations at the Royal Observatory at Greenwich, and indicates that a correction of the elements is required.

		$\cos \delta . da$	$d\delta$
1857	February 27	+ 1'05	+ 4'29
1858	June 10	+ 24'44	- 0'28
1859	September 21	- 10'44	- 6'40
1861	January 13	- 31'93	- 15'85
1862	April 26	+ 3'55	+ 7'05
1864	November 5	- 36'36	- 24'76
1866	January 23	- 55'14	+ 3'68
1867	May 24	- 16'03	+ 13'28
1868	September 3	- 41'85	- 12'64
1869	November 9	- 77'01	- 37'66
1871	March 22	- 55'99	+ 30'64
1872	July 2	- 43'77	+ 10'67
1873	September 25	- 73'57	- 39'29
1875	January 24	- 107'89	- 22'92
1876	April 30	- 61'46	+ 38'07

EQUATIONS OF CONDITION.

Substituting their Logarithms for the Coefficients, and (dn) for 100 $\dot{dn}$.

	<i>Right Ascension.</i>			
	$d\epsilon$	(dn)	$d\pi$	$d\phi$
0 = +0.02167	+0.25343	-1.82727	-9.42959	-8.74531
0 = +1.38815	+0.15866	-1.67341	+8.79309	+8.71883
0 = -1.01880	+0.06763	-1.51698	+9.28063	-8.48159
0 = -1.50417	+0.13935	-1.51390	-8.88258	-8.47363
0 = +0.35082	+0.18784	-1.46086	-9.23279	+8.80257
0 = -1.56059	+0.06328	-1.04516	+9.06618	+8.59062
0 = -1.74145	+0.26956	-0.95626	-9.41007	+8.62961
0 = -1.20496	+0.16748	-8.81601	-8.10106	-8.68744
0 = -1.62167	+0.08108	+0.73824	+9.30775	+8.89115
0 = -1.88653	+0.15478	+1.11607	-7.75740	-8.70851
0 = -1.74809	+0.22490	+1.36820	-9.35468	+8.85327
0 = -1.64113	+0.14550	+1.41454	+9.08732	-8.32243
0 = -1.86670	+0.07492	+1.44078	+9.19673	+8.15594
0 = -2.03298	+0.20767	+1.64983	-9.19207	-7.22272
0 = -1.78857	+0.18405	+1.69760	-9.01030	+8.24304
				+8.83916
				+9.61091
				+8.26269
				+9.74006
				-8.47363
				+9.78662
				+9.74328
				+9.00651
				+9.34380
				+9.58870
				+9.41996
				+9.80386
				-8.79560
				+9.80866
				-8.99167
				+9.69184

Declination.

	$d\epsilon$	(dn)	$d\pi$	$d\phi$
0 = +0.63246	-9.69624	+1.27768	+8.87221	-8.97859
0 = -9.44716	-9.60969	+1.12470	-8.23502	-9.45291
0 = -0.86618	+9.66285	-1.12072	+8.89691	+7.91116
0 = -1.20003	+9.70228	-1.07136	-8.52930	-9.40821
				+0.14955
				+8.81178
				-0.13094
				+8.93490

0 = +0.84819	-9.88424	+1.16443	+8.96774	-9.96629	+9.12824	+0.08078
0 = -1.39375	+9.75563	-0.75036	+8.80393	-9.95905	-9.13716	-0.03589
0 = +0.56585	-9.23843	+0.02674	+8.29688	+9.41652	-9.33933	+0.02312
0 = +1.12320	-9.78392	+9.10522	+7.98811	-0.07942	+9.41450	+9.72902
0 = -1.10175	+9.53698	+0.11943	+8.76664	+9.10233	+8.92758	-0.11871
0 = -1.57588	+9.76484	+0.71235	+7.68931	-0.06718	-9.38773	-9.80866
0 = +1.48629	-9.86102	-0.98924	+9.01481	-9.48451	+8.29070	+0.15899
0 = +1.02816	-9.31789	-0.59749	-8.33385	-9.51965	+9.43678	-9.62017
0 = -1.59428	+9.75783	+1.11470	+8.91566	-9.74813	-8.74013	-0.11989
0 = -1.36021	+9.48048	+0.93313	-8.54506	-9.54760	-9.40890	+9.76424
0 = +1.58058	-9.88245	-1.39200	+8.77685	-0.10034	+9.30107	+9.99529

Reducing by the method of least squares, the final equations are

0 = -3.00135	+1.56490 $d\epsilon$	-1.81720 (dn)	-0.08089 $d\pi$	+0.40170 $d\phi$	-9.79403 $d\nu$	+9.20734 di
0 = -4.15314	-1.81720	+4.27147	+0.93347	+1.15519	+0.22440	+0.26440
0 = +1.37076	-0.08089	+0.93347	+9.65020	-9.44460	-8.33700	-7.84111
0 = +2.73637	+0.40170	+1.15519	-9.44460	+1.85709	-8.77328	+9.52970
0 = +1.45517	-9.79403	+0.22440	-8.33700	-8.77328	+9.77366	+9.27225
0 = +1.69038	+9.20734	+0.26440	-7.84111	+9.52970	+9.27225	+1.25074

whence

$$d\epsilon = +29.26 + dn \times t$$

$$d\pi = +3.13$$

$$d\nu = -19.73$$

$$\begin{aligned} di &= -2.73 \\ d\phi &= -8.76 \\ dn &= +0.00871 \end{aligned}$$

t being the number of days from 1867 May 24.0 G.M.T.

RESIDUAL ERRORS.

		Equations.		Direct Calculation.	
		$\cos \delta \cdot da$	$d\delta$	$\cos \delta \cdot da$	$d\delta$
1857	February 27	-4.8	+2.5	-4.8	+2.5
1858	June 10	-0.1	+0.6	-0.5	+0.6
1859	September 21	+1.1	+0.7	+1.1	+0.7
1861	January 13	+1.1	+1.1	+1.0	+1.3
1862	April 26	+2.4	-0.1	+2.3	-0.1
1864	November 5	+2.7	+0.8	+2.9	+0.8
1866	January 23	+4.2	-1.2	+4.3	-1.1
1867	May 24	-0.8	-0.4	-0.9	-0.3
1868	September 3	-1.2	-0.4	-1.2	-1.1
1869	November 9	-1.0	+0.6	-0.4	+0.7
1871	March 22	+1.3	-0.5	+1.4	-0.6
1872	July 2	-1.0	-0.3	-0.9	-0.1
1873	September 25	-3.7	-1.4	-4.0	-1.5
1875	January 24	-1.5	-0.1	-1.4	0.0
1876	April 30	+0.3	-1.1	+0.3	-1.4

The Centre of Gravity of the Apparent Disk of a Planet.

By Prof. Asaph Hall.

When a planet is observed from the Earth its disk will not be completely illuminated except at the time of opposition. At other times it will appear gibbous or crescent. If we assume the planet to be a sphere, which, according to the best observations is the case with *Mercury*, *Venus*, and *Mars*, the apparent disk will be composed of a semicircle and a semi-ellipse, which are separated by the line of cusps. If a right line be drawn through the centre of the line of cusps and at right angles to it, the centre of gravity of the apparent disk is in this line. In the plane triangle between the Sun, the Earth, and the planet, let ϕ be the angle at the planet, let a be the radius of the semicircular part of the disk, and denote by m the distance of the centre of gravity from the centre of the line of cusps. Then, when the planet is gibbous we shall have

$$m = \frac{8a}{3\pi} \sin^2 \frac{1}{2} \phi,$$

and when the planet is crescent,

$$m = \frac{8a}{3\pi} \cos^2 \frac{1}{2} \phi,$$

where π is the number 3.1416. The angle ϕ and the radius a can be computed from the data given in the Ephemerides, and the distance m is known, therefore, for any time.

In order to show the use of the quantity m , suppose the angle of position and distance of a satellite of a planet have been observed with a filar micrometer in the following manner. The wire is laid across the apparent disk of the planet in such a way that this disk is divided into two parts of equal area. The wire therefore always passes through the centre of gravity of the apparent disk, and it is necessary, before using the observations for computing orbits, to reduce these measures to the true centre of the planet. If we denote by p and s the observed angle of position and the distance of the satellite, and by θ the angle of position of the line of cusps, the errors of p and s arising from the unsymmetrical form of the disk will be

$$\Delta s = m \cdot \sin (p - \theta),$$

$$\Delta p = \frac{m}{s} \cdot \cos (p - \theta),$$

where Δp is expressed in parts of radius.

To complete the solution we have to find the value of θ . This is given from the spherical triangle between the pole of the Equator and the geocentric places of the Sun and the planet. In this triangle the angle at the planet is $90^\circ - \theta$; and if we denote by d the angular distance between the Sun and the planet, by $\alpha, \delta, \alpha', \delta'$ the geocentric right ascensions and declinations of the Sun and the planet, we shall have

$$\cos d = \sin \delta \sin \delta' + \cos \delta \cos \delta' \cos (\alpha' - \alpha),$$

$$\sin d \cos \theta = \cos \delta \sin (\alpha' - \alpha),$$

$$\sin d \sin \theta = \sin \delta \cos \delta' - \cos \delta \sin \delta' \cos (\alpha' - \alpha).$$

These equations give the angle θ without ambiguity.

In the case of a spheroidal planet, as *Jupiter* or *Saturn*, the apparent outline of the planet will be composed of ellipses, but the determination of these ellipses, though possible, is much more troublesome, and for these planets the correction for defective illumination is hardly sensible.

Washington, 1877, November 15.

On a Portable Equatoreal Mounting for Small Telescopes.

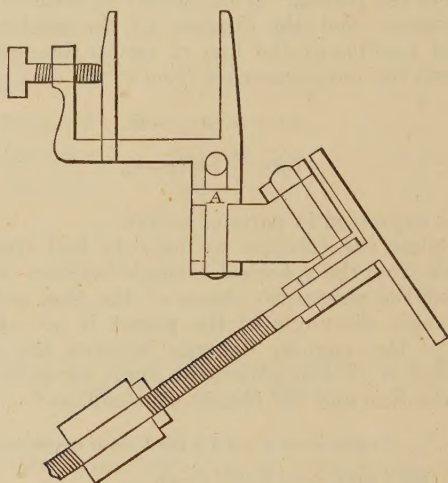
By Wentworth Erck, Esq.

This mounting is adapted for travelling astronomers or seaside visitors.

That now shown has been found quite strong enough to carry a $3\frac{1}{2}$ -inch telescope of 50 inches focus, yet the mounting packs into a space of $9 \times 4\frac{1}{2} \times 1$ inch, and weighs less than 3 lbs.

It is evident that it can be attached to any window sash, either French or English; or to any post or rail, whether vertical or horizontal; and it will not injure a painted surface.

If it be attached to an English window sash, the sash must first be steadied by inserting a small wedge between the sash and the window frame. In this way it will give a far more extended and more comfortable view than the ordinary tripod stand.



When the mounting is to be attached to a vertical post, or French window sash, then the vertical axis A is to be unscrewed out of the cubical block, and screwed into another face of the cube at right angles to the former face.

The mounting consists of a small vice, from which depends a vertical axis, on which turns the frame carrying the polar axis, so as to admit of adjustment in azimuth.

The present mounting being intended for use between London and Dublin only, it was not thought necessary to make any provision for altering the inclination of the polar axis. This can be done, if desired, by introducing another joint between the polar and vertical axes. All the screw-heads and nuts are of the same size, so that they can be tightened by the same wrench.

1878, January 9.